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Reply to comment by Yanni Gunnell and Marc Calvet on "Origin of the highly elevated Pyrenean peneplain"

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[1] *Gunnell and Calvet* [2006] (hereinafter referred to as GC) challenge the recent model that we proposed for the origin of the highly elevated Pyrenean peneplain by contesting our morphometric analysis of this chain and the relation we made between the morphological evolution and the piedmont sedimentation. Their reasoning is as follows: (1) According to *Calvet* [1996] (on which their comment is largely based) the high-elevation, low-relief surfaces in the Eastern Pyrenees are remnants of a peneplain that developed before the Pliocene from applanation near to sea level, and which was later uplifted by ~2000 m during the Plio-Quaternary (in other words, GC belong to the ''applanation'' school, whereas we would belong to the ''altiplanation'' school); (2) high-elevation, low-relief surfaces do not exist in the Central Pyrenees; (3) therefore the relationships we made between the morphology of the Central Pyrenees and the pattern of the detrital sedimentation in the adjacent Ebro foreland basin is meaningless; (4) contrary to the initial interpretation of *Calvet* [1996], GC recognize that crustal thickening did not develop since the Pliocene in the Eastern Pyrenees, so they appeal to another geodynamical process such as extension or lithosphere delamination to explain the supposed uplift.

[2] First, we make two points: (1) our model is not only based on observations from the Central Pyrenees but it also includes the Eastern Pyrenees and, in particular, the detailed morphology of the Cerdanya area, the analysis of which GC do not contest; (2) the distinction between the central and Eastern Pyrenees is geographical but in no way reflects a major structural zonation of the axial zone, except from the occurrence of localized Oligo-Miocene transtension in the Eastern Pyrenees, a point we will address later.

[3] The surfaces that were identified by *Calvet* [1996] in the westernmost part of the Eastern Pyrenees are immediately adjacent to the ones we identified in the Encantats. It is true that we do not prove directly that the Encantats surfaces are pre-Quaternary, but this is also the case for the surfaces in the Eastern Pyrenees except in the Cerdanya area [see *Babault et al.*, 2005, section 4.1.]. It is patently obvious [see also *de Sitter*, 1952] that glaciers have developed on previous erosional surfaces during the Quaternary both in the Central and the Eastern Pyrenees. However, that the Pyrenean high-elevation, low-relief surfaces resulted from glacial erosion is untenable. If it were the case, then any topographic profile across Alpine-type chains at midlatitudes should show a drastic decrease in the local relief with increasing mean elevation as in the Pyrenees. This is not the case as shown in Figure 1. We are also aware of the geology of the Central Pyrenees that consists of granites and Paleozoic metasediments, with the latter being affected by tight upright folds and pervasive vertical schistosity, so that possible confusion between structural and erosional surfaces as stated by GC is unfounded. The rest of their remark concerning the confusion we could have made between high-elevation, low-relief surfaces and glacial paleovalleys is also unfounded as they do not provide any contradictory data to our observations. Our interpretation of the morphology in the Encantats massif agrees with the observation of *Calvet* [1996] in the same area that these erosional surfaces merge in the wide glacial flat bottomed cirques [*Calvet*, 1996, pp. 346, 1116]. We are not aware of any published work that refutes this observation of Calvet.

[4] We use the term peneplain as many authors before us to describe a landscape of low relief resulting from prolonged subaerial erosion, according to the definition given by *King* [1953] [see also *Philipps*, 2002]. Finally, we agree with *de Sitter* [1952], who wrote ''By the end of the Miocene, the Pyrenees represented therefore a gently undulating, very mature landscape, almost a peneplain with low hills, which in the center do not rise above 1000 m altitude.''

[5] The second major point that GC raise concerns the relationship we made between the existence of high-elevation, low-relief surfaces and the adjacent huge detrital sedimentation in the Ebro basin. First, our interpretation does not require the existence of any Pyrenean ''Gangplank,'' but it implies that the erosional surfaces were the source area of the detrital sedimentation. By definition, recovering of these surfaces with detrital sediments just means that the surfaces develop before sedimentation, as is

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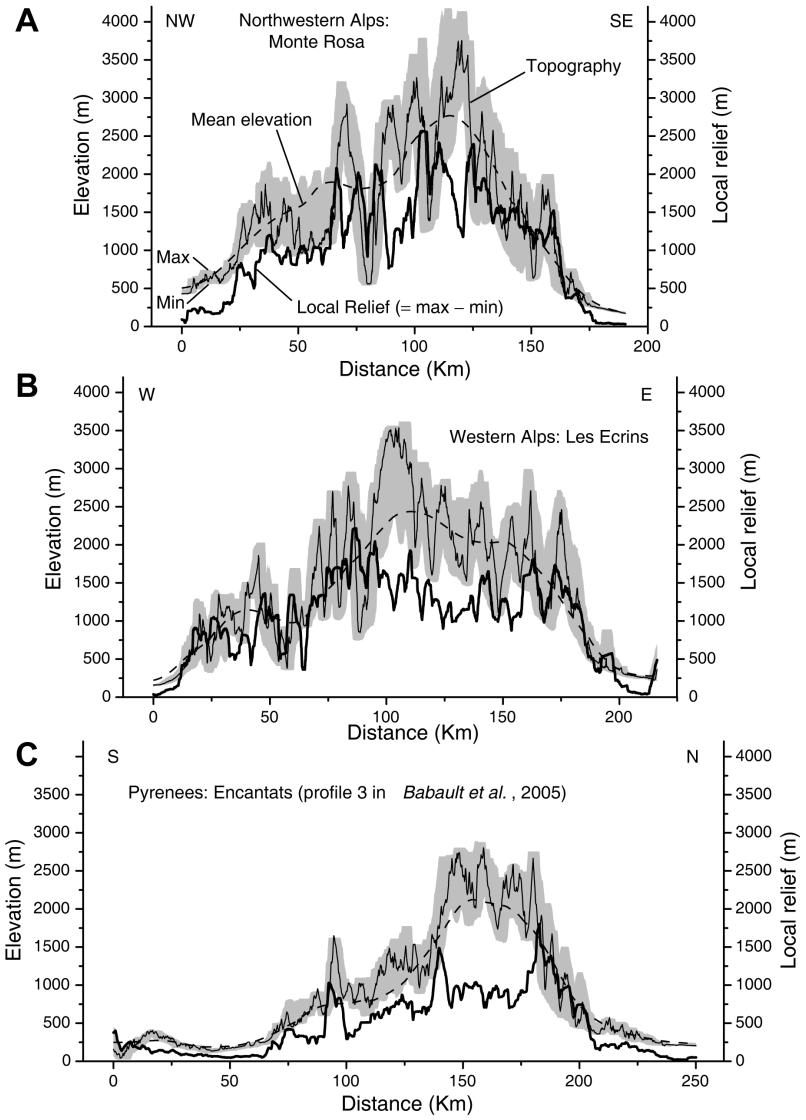


Figure 1. Relationship between mean elevation and local relief in the western Alps and the Pyrenees. (a) Northwest to southeast (7°1′/46°54′; 8°23′/45°29′) topographic profile across the northwestern Alps. (b) West to east (4°57′/44°57′; 7°43′/45°02′) topographic profile across the western Alps. (c) South to north topographic profile across the Central Pyrenees. The three profiles have been performed using the same method [see *Babault et al.*, 2005]. Note that along the profiles across the Alps a decrease in the local relief can be observed in some places where the mean elevation is high, indeed possibly due to the occurrence of glaciers, but none of these profiles shows the particular pattern observed in the Pyrenees, precluding a glacial origin for the high-elevation, low-relief surfaces seen in the Central Pyrenees.

the case in the Cerdanya area, allowing these surfaces to be dated as pre-late Miocene. So the fact that no Gangplank exists in the Pyrenees is meaningless. Second, we strongly disagree with GC's unsupported assertion that the conglomerates contain no pebbles from the Encantats. The reference to *Reille*'s [1971] is misleading because he only suggested a more westerly origin, and this is contradicted by more recent work [*Vincent and Elliott*, 1997].

[6] To challenge the slope value that we used to fit the piedmont to the high-elevation, low-relief surfaces in our Figures 4 and 5 [*Babault et al.*, 2005], GC appeal to the slope values of alluvial fans given by *Stanistreet and McCarty* [1993]. The values for the slopes of alluvial fans are a topic of debate. We note that *Blair and McPherson* [1994] and *Smith* [2000] have challenged the classification of *Stanistreet and McCarty* [1993]. *Smith* [2000] shows that in the case of streamflow dominated piedmont the slopes of alluvial streams are commonly steep with values in the range 0.5°–2.0°. Quaternary piedmont slopes of 2.5°–2.85° over distances of 20–25 km have also been described along northeastern Tibet [*Meyer et al.*, 1998; *Mériaux et al.*, 2005]. Therefore GC's claim that the slope values we used are not correct is unjustified, and the 1–2 km post-Oligo-

cene tilting that they invoke as an alternative explanation remains to be proved.

[7] We do not discuss here point four of GC's comment concerning detrital sedimentation since we already discussed this extensively in section 6.2 of our original paper [*Babault et al.*, 2005]. GC do not provide any new point or evidence against our observations and interpretations, and we can only invite them to read the original paper again.

[8] The third major point from GC concerns the need for extension or slab detachment to explain the supposed 2000 m uplift in the Eastern Pyrenees. This is a surprising criticism, as we already discarded this hypothesis [*Babault et al.*, 2005, paragraph 3].

[9] We strongly disagree with GC when on the basis of *Vacher and Souriau* [2001] and *Sibuet et al.* [2004], they invoke the possibility of slab detachment to account for postorogenic uplift. Neither of these studies questioned crustal shortening in the Pyrenees as stated by GC, on the contrary, they agree with the deep structure of the lithosphere which is well known to be the simple result of continental subduction [*Choukroune and ECORS Team*, 1989; *Daignieres et al.*, 1989]. If the detachment slab process had ever occurred, it must have been surprisingly overcompensated by a high-density crustal root.

[10] In the same way, extensional tectonics as invoked by GC cannot explain such uplift in the Pyrenees. Indeed, extension results essentially in surface collapse, except, at rift margins or if it involves a strong thermal anomaly. The main extensional event in the Eastern Pyrenees developed during the Oligocene-Miocene. There is no evidence that a major thermal anomaly developed afterward below the Pyrenees. The current surface heat flow below the northern and southern Pyrenees is 69 ± 10 and 50 to 70 mW/m^2, respectively [*Lucazeau and Vasseur*, 1989; *Banda et al.*, 1991].

[11] Eventually, (1) the observation of the present-day low elevation (∼60 m above sea level [*Gállego et al.*, 1983; *Mató Paló et al.*, 1995]) of the early Pliocene discordance between the lower marine series and the upper continental sediments in the easternmost part of the Pyrenees and the Ebro basin (Empordà area) and (2) the absence of major normal faults precluding any decoupling of these areas from the rest of the Pyrenees, both demonstrate that no major uplift occurred during Pliocene-Quaternary times, neither in the Eastern nor in the Central Pyrenees, and thus certainly not in the Pyrenees as a whole. GC's reference to the Rocky Mountains where extension and related mantle thermal anomaly may explain, in part, surface uplift is therefore misleading. This leads us to suggest in return that piedmont sedimentation might have played an unsuspected role in the development of high-elevation, low-relief surfaces in this area.

[12] GC provide no evidence for any geodynamic process that could have resulted in the 2000 m uplift of the pre-Pliocene near sea level peneplain in the Eastern Pyrenees. Neither do they provide any substantial argument to attribute these surfaces in the Eastern Pyrenees to aerial erosion and the immediately adjacent similar surfaces in the Central Pyrenees to glacial erosion, the distinction between Eastern and Central Pyrenees being essentially geographic. As we wrote in our original paper, the detrital sediments of the Ebro basin onlap onto the Paleozoic series of the Axial Zone in a backstepping manner, and there is no major tectonic discontinuity along the southern flank of the Pyrenees that could explain the relative uplift of these detrital sediments with regard to the Axial Zone. Therefore the present-day small difference in elevation between the erosion product of the chain (conglomerates) and the source area in the Axial Zone reflects the progressive rise of the base level of the chain.

[13] Finally, readers of GC's comment will see that it essentially brings assertions and refutations which are unsupported or are contradictory with existing data and in particular, with *Calvet*'s [1996] previous work. However, we welcome their remark on the necessity of carefully taking into consideration detailed geologic data and field geomorphology but without neglecting geophysical data. Indeed, we believe that modern geomorphology requires considering both deep lithospheric and surface processes because any surface uplift originates at depth in the Earth. We are convinced that doing this will greatly help geomorphologists to no longer consider reality as a "chicken omelette," insofar as the ultimate goal is to distinguish the cause from the effect.

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